

True Peer Review

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Abstract—In computer science, conferences and journals conduct peer review in order to decide what to publish. Many have pointed out the inherent weaknesses in peer review, including those of bias, quality, and accountability. Many have suggested and adopted refinements of peer review, for instance, double blind peer review with author rebuttals.

In this essay, I argue that peer review as currently practiced conflates the sensible idea of getting comments on a paper with the irrevocably-flawed one that we either accept or reject the paper, which I term *gatekeeping*. If we look at the two separately, then it is clear that the ills associated with current peer review systems are not due to the practice of getting comments, but due to the practice of gatekeeping.

True peer review constitutes my proposal for replacing existing peer review systems. It embraces the idea of open debate on the merits of a paper; however, it rejects unequivocally the exercise of gatekeeping. True peer review offers all the benefits of current peer review systems but has none of its weaknesses. True peer review will lead to a truly engaged community of researchers and therefore better science.

I. THE DEBATE ON PEER REVIEW

“You just have to resubmit and hope to get assigned the *right* set of reviewers,” advised an experienced mentor with whom I was discussing ways of improving a recently rejected paper of mine. In other words, *keep trying until you get lucky*. Whereas the advice was well-meaning, it betrayed a lack of trust in the peer review process. Others have echoed a similar sentiment. Naughton [6] in a recent well-publicized keynote mentions the large role a lucky assignment of reviewers plays in getting a paper accepted. Anderson [1] backs up this claim with statistical evidence from computer systems-related venues.

Many have noted the problems with traditional peer review. Casati et al. [2] criticize the current publication model for entangling the separate concerns of dissemination, evaluation, and retrieval. In his keynote, Naughton noted the problems resulting from the combination of the pressure to publish, low acceptance rates, and poorly-trained reviewers, including that of undue negativity in reviews. More commonly noted are the problems of bias and accountability and that most speculative, potentially interesting research tends to get rejected in favor of incremental work [3]. A survey of researchers undertaken on behalf of the Publishing Research Consortium (PRC) contains extensive pointers to the ongoing debate on the efficacy of peer review [10].

Researchers in computer science recognize some of the limitations of peer review, and they are changing their systems to mitigate them. For example, the AAMAS (Autonomous Agents and Multiagent Systems) series of conferences implement double blind paper reviewing with author rebuttals.

Further, senior program committee members and the program chairs monitor the quality of the reviews. Some conference series such as ICSE (Software Engineering) and RE (Requirements Engineering) have separate tracks for vision papers and new ideas and emerging results. The VLDB Foundation no longer publishes conference proceedings: all papers accepted to the foundation’s journal are presented at the next VLDB conference. Some have adopted more open systems of peer review in order to counter the problem of accountability, for example, the now defunct Electronic Transactions on Artificial Intelligence. One of Naughton’s proposal for improvement is not conducting peer review at all and accepting everything. Besides the engineering of peer review systems, researchers have also attempted to educate potential reviewers and writers on their respective tasks [9], [7]. Reviews forms at most conferences and journals are increasingly detailed, ostensibly to make sure that reviewers not overlook any major quality of the paper.

While these are all well-intentioned efforts, they miss the point: we *must* get out of the accepting-rejecting business altogether and instead embrace the true spirit of scientific engagement, which I term *true peer review*. The rest of this essay is an elaboration of this point.

II. GATEKEEPING

In computer science, conferences and journals conduct peer review in order to decide what to publish. Conferences and journals are, in effect, institutions that perform the function of *gatekeeping*: the intent is to let in only good work. What passes the gate is *published*. Lest we get too hung up on adjectives, you can replace “good” with your favorite adjective, e.g., “interesting”, “original”, “solid”, and so on.

Implicit in gatekeeping is the notion that what is published is *authoritative*: worth knowing, worth citing, and worth building upon. What is not published is not worth knowing. (No conference or journal that I know even publishes a list of rejected papers.) Not published means low quality. Conversely, published means high quality. Without these two axioms, gatekeeping would lose much of its legitimacy. The axioms may sound extreme but when I look around I see most people and institutions behaving as if they were true. Consider for instance that one is neither likely to get tenure nor any research funding without having published substantially. Or consider, for instance, that authors are not likely to cite anything except published work. Recently, I was criticized for citing workshop papers, presumably because they are not as rigorously peer-reviewed. Consider, for instance, that the recipe for success

that is most freely dispensed to junior researchers and faculty these days is not “explore this theme deeper; it will lead to good results”; it is “publish a lot in the top venues”. Others before me have put it more succinctly: *publish or perish*.

This would all be well and good if gatekeeping were working. There are two problems with gatekeeping. Whereas one is conceptual and therefore more fundamental, the other concerns the way gatekeeping is currently practiced.

A. The Problem of Demarcation

Demarcating the good science from the bad is an enormously difficult task. In fact, if the philosophy of science has shown us anything, it is that such value judgments are bound to be subjective: the review depends on the reviewer [5]. Each reviewer’s intellectual biases will inform his or her reviews.

We are all intellectually biased and our biases run so deep that we may not even recognize them as such. We all have our own inspirations, our own beliefs, our own inclinations, and our own favorite theories. We all have different research backgrounds, with some of us having worked in competing research paradigms. We all apply subtly different evaluation criteria by which we judge research. We potentially favor different styles of exposition. Our emotional attitudes are also different; for instance, some of us may be more forgiving of errors than others. When one reviews a paper, he or she brings all this to bear upon the review, but for the most part only tacitly. And yet we are inclined to claim objectivity!

We already know how deeply subjective peer reviewing is. We know this because different reviewers give different ratings for the same paper. In fact, often enough, the reviews are blatantly conflicting. Of course, even with conflicting reviews, gatekeeping means that a decision must be made. So reviewers are encouraged by editors to resolve their differences so that when the final decision goes out to the authors, it would appear to have had unanimous support. When reviewers stick their ground, additional reviews may be solicited. Then based on the reviews and the discussion, the program board (it could be just the editor or program chair) somehow makes a judgment call. All this simply goes to highlight the subjectivity of peer review. It also goes to show how hard conferences and journals try to create the illusion of objectivity where none exists.

Forget conflicting reviews. Consider the case of unanimity. Do three favorable reviews mean the work is objectively good and three unfavorable ones that it is objectively bad? If three other people were to review the work, couldn’t a “good” verdict turn “bad” and vice versa? If the whole world were to vote ‘bad’, it would likely have social and psychological consequences for the authors, but that still wouldn’t make his or her paper bad.

That we are all intellectually biased is not a bad thing; it is simply the way we are. Kuhn, in fact, paints our intellectual biases in a relatively positive light by explaining their value for problem solving within a research paradigm [4]. Our subjectivity is to be celebrated, not bludgeoned to death by having us apply supposedly “objective” criteria to judge the merits of research.

The simple point is that that while informed subjective viewpoints may be valuable, they cannot serve as the means for objectively separating the good from the bad. Gatekeepers have taken upon themselves an impossible task. In fact, if we accept our subjectivity, then gatekeeping actually gets in the way of progress. There is simply no need to put works published in conferences and journals on a pedestal at the expense of others.

B. The Problem of Accountability

Peer review processes inspire little confidence in the authors because no one on the reviewing side is accountable to the authors—not the reviewers, not the program boards and program chairs at conferences, and not the editors at journals. The reviewers are anonymous and the authors are not privy to any of the discussions on the reviewing side. From the time that the author submits a paper to the time a decision is made, he or she is completely out of the loop.

The authors may get a chance to respond to the reviewers but that seldom has any effect. With journals, there can be more of a back and forth, but it is still quite limited. The authors may complain to the program chair or editor but that too rarely has any effect except for a courteous reply saying the he or she must follow the recommendations made by the reviewers.

Why is accountability important? Some people believe that reviews by and large are of good quality, but my experience as author, reviewer, and program committee member has been to the contrary. Many reviewers simply repeat the authors claims followed by what would seem like an arbitrary decision; many give all kinds of flimsy reasons that, if not thoroughly unscientific in attitude, have nothing at all to do with the contents of the paper. Many reviewers simply follow a lexical pattern-matching algorithm when doing a review (conferences and journals, as mentioned before in Section I, provide templates for writing reviews). For example, there are those who are so obsessed with experimental validation that I believe that they would have told Dijkstra that his solution to the Dining Philosophers Problem was wholly impractical because philosophers are known to be an unruly bunch in general.

Further, interpersonal relationships and other psychological factors likely play a big role in gatekeeping. Aren’t reviewers stung by direct criticism and pleased by praise of their own work? How common is it that reviewers will recommend rejection of papers that take positions contrary to their own and accept those that praise, extend, or complement their own? How commonly does the reputation of the authors bear on the decision? Don’t many reviewers write their reviews in a hurry? Don’t many (including people such as program chairs and journal editors) not to want to revisit their original reviews because of the extra work involved? People want to be on program committees and editorial boards but do they want to write reviews? Coupled with the fact that conferences want to have low acceptance rates, doesn’t the fact that many of the reviewers are also authors produce a serious conflict of interest? Many conferences and journals say their review

processes are rigorous and fair, but they mean these things only in a very narrow bureaucratic sense, that is, the steps of the peer review process were followed.

Good conferences and journals in computer science have acceptance rates ranging from 15-25%. If we find a vast majority of the papers unacceptable, why do we think that the vast majority of reviews would be acceptable? In my experience so far, editors and program chairs are prone to turn a blind eye to author complaints about unfair reviews; they justify doing that by saying the decision was arrived at following due process. How many times are reviews overturned?

III. TRUE PEER REVIEW

What people currently understand as peer review conflates two things: getting comments on a paper and gatekeeping, that is, deciding whether it should be accepted or rejected. The first is valuable and can potentially lead to improvement in the paper. *The latter is just a case of getting our priorities wrong.* Right now, we think that we must build an authoritative source of knowledge, therefore we must do gatekeeping. Peer review is the means gatekeepers use to justify their ends. The first step towards solving the problems of what is currently understood as peer review is to recognize gatekeeping and peer review as two different activities. For clarity, I term the activity of peer review untangled from gatekeeping as *true peer review*.

True peer review is an exchange of informed opinions on a paper. It happens in a community of scientists. True peer review is an argumentative setting that also actively involves the author. It recognizes that comments, discussions, and arguments can potentially lead to improvements in a paper. It may be formal or informal. We often engage in true peer review over email when we send drafts and papers to others for comments. We participate in peer review when we ask or get questions at research seminars. Can anyone claim that the reviews obtained from unaccountable anonymous randomly assigned people are better than the questions and comments you receive from those whose opinions you value and sought? More formal true peer review systems will involve third-party repositories of papers and discussions about them.

Argumentation is the mechanism of true peer review. It is likely that no consensus would be achieved even after vigorous argumentation but this is not a problem because even the exploration of different points of view in an activity valuable in itself. For a researcher, the record of arguments could turn out to be just as rich a source of information as the paper being argued about. This record is as valuable a scientific document as the paper it comments on. It is a pity that reviews from the current peer review system are for all practical purposes *forgotten*.

To engage in true peer review is up to individuals, whether in the role of a reviewer or author. Nobody can be forced to engage in it. For example, an author may choose to not solicit any reviews; and potential reviewers may turn down requests from the author. However, that hardly discredits true peer review. If people tend not to write reviews voluntarily, there is no reason to think they would do a good job of it

if forced. If people don't solicit reviews on their work, that doesn't stop anyone from either ignoring it or remarking upon it in their own work.

Naturally, in any argumentative setting some participants may wield more influence than others, but that is no different than current peer review systems. Unlike current peer review though, true peer review is an end in itself, not the means to the irrevocably-flawed notion of gatekeeping.

IV. FALSE ARGUMENTS

Below, I go through a list of arguments that ostensibly make the case for gatekeeping; however, I show them all to be false.

Reviewers at conferences and journals are more knowledgeable than the authors. If we could make objective claims about who is more knowledgeable, we would be able to settle conflicts among reviewers by that criterion. We wouldn't even need three reviewers—just one reviewer more knowledgeable than the author would suffice. The simple fact is we can't settle any difference of opinion by resorting to claims of knowledgeability. Further, consider that in practice, papers are often reviewed by junior researchers, including doctoral students (not a bad thing in itself, but it does undermine the claim to authority).

Without gatekeeping, there would be no authoritative source of knowledge. Where would one even begin to look for information? If by 'authoritative', one means worth-knowing, I already substantively dismissed that argument in Section II. Gatekeeping produces results informed by personal biases, both intellectual and political. In fact gatekeeping makes things worse: people will potentially restrict themselves to a narrow body of published work.

One can turn this question around and ask if one must consider everything that anyone bothers to write on a topic. How can one possibly cope? The simple answer is yes: ideally, one must do that anyway. Should a researcher not consider an unpublished report simply because it was unpublished? Should he or she not consider it because it was written by some hitherto unknown person? Should he or she not consider it because it is only four pages long instead of ten? It is the *ethical responsibility* of a researcher to consider everything regardless of whether published or not or who the authors are or what the format is. In fact, it is researchers who attach a high value to gatekeeping who are unlikely to meet this ethical responsibility.

Even now we find a lot of information through our social networks, which includes advisers, colleagues, collaborators, students, and so on, and through Web search. We often ask experts for references. A novel idea of dissemination that came out of the LiquidPub project (<http://liquidpub.org/>) was that a researcher could publish his or own journal. The journal would contain papers written by others that the researcher thought worth perusing, perhaps with his or her own comments. Journals published by experts would probably be more visible than those published by relatively unknown researchers (a precedent of this idea can be found in Dijkstra's unpublished manuscripts). I am confident that without gatekeeping, new

efficient ways of finding and keeping track of information will emerge.

Let me ask those who are frightened by what seems to them an immense overload of information in a true peer review world: do they read every relevant paper published in conferences and journals? If they don't anyway, why bother to raise it as an argument against true peer review?

Getting your paper published or not published is a choice. One is free to not submit papers anymore to peer-reviewed venues. As pointed out in Section II, given the extraordinary importance accorded to published work, one does not really have a choice. True freedom is not about having choice, but about the freedom free from pressure to exercise this choice.

The process of gatekeeping has produced many influential publications. There is no evidence for the claim. I could argue that the publications were influential only because they were published or that they would have been influential even without gatekeeping.

Given that people are biased anyway, we can't do much better than gatekeeping. There is no evidence for this claim. A counter-argument is that gatekeeping institutionalizes personal biases. In other words, instead of a person saying that "I like (do not like) this paper", it is the institution (recall that by 'institutions', we mean conference and journals) that says "We accept (reject) this paper".

Let personal biases be personal; a person can choose to make his or her biases known in comments and reviews. But there is no reason to make unaccountable anonymous reviewers' personal biases institutional. Even if the reviewers' identity were made public, recall from Section II-A that reviewers are not authoritative sources of knowledge.

But how can we judge the performance and merit of researchers without turning to publications? How can we make hiring and promotion decisions? I don't have a definite answer but I don't think that the lack of gatekeeping makes these tasks any more challenging. For example, let's consider the task of hiring someone. How would we do that? By reading a few samples of what he or she has written, by paying attention to the presentation of his or her work, by listening to his or her vision of the future, by asking questions, by interacting, by probing the depth and breadth of his or her understanding, by judging his or her passion, by judging how well he or she articulates his or her thoughts. One cannot evaluate a candidate on the basis of the broken system that is gatekeeping. I think hiring decisions already consider most of the above factors, but publications are likely given a weight higher than any other factor (hence the mantra *publish or perish*), which I think is misguided.

Consider that the current criteria for hiring have emerged because of the importance we give to publications. If we had no gatekeeping, likely some other set of criteria would emerge. One may argue that looking at the publication record simplifies a difficult decision, but that is an optimization given that our overriding *value* is to spend as little time and effort as possible on these decisions. If that value persists, the criteria that would emerge in the absence of gatekeeping would also be in keeping

with that value.

Given the limited time and resources at conferences, how do we decide which papers to present and which to not? This is a separate logistical problem similar to the one about hiring and promotions. We may have to rethink how we do conferences. Perhaps there should only be poster sessions at conferences. Let researchers work on attracting audiences to their posters; let them go and actually *talk* to others about their work rather than just do a 20-30 minute presentation that few in the audience are interested in. Solutions to logistical problems will emerge.

Publications are incentives for researchers to produce high quality work. That seems like an absurd claim. The best researchers are driven by passion and the inklings of a superior way of doing things. They want to disentangle the threads, connect the dots, fill the gaps, and turn things inside out. It takes gatekeeping (and systems of evaluation based upon it) to make a good researcher produce bad work.

Personal biases, both intellectual and political, will not be eliminated in true peer review. True. However, the bias is no longer institutional. There is no censorship.

True peer review cannot guarantee that every paper receives comments or reviews. True. But balance that against the fact that not all reviews one gets in the traditional system are useful. True peer review encourages you to seek comments from people who *you* think could provide useful comments. True peer review encourages people to engage with each other in meaningful discussions. When researchers discuss the relevant literature in their own report, they are engaging in a limited form of true peer review (although, unfortunately, the discussion of the literature is often a mere formality in practice because of the political dimension of gatekeeping).

As mentioned before, one could set up online repositories of papers where people could comment and carry out debate on the merits of papers therein. Additionally, one could set up incentives to encourage people to review the papers published there. Comments could be read and rated by others. The best comments would filter to the top and provide the commentator visibility. The point is *we can engineer systems to support true peer review*.

V. PRAGMATIC BENEFITS OF TRUE PEER REVIEW

- True peer review saves time. One does not have to make the changes one feels unnecessary merely to satisfy reviewers. Authors can do the changes they feel necessary and move on to the next thing. One will have more time and energy to pursue his or her own passions instead of being caught up in the revise and resubmit until accepted cycle.
- Bid adieu to *publish or perish*. Since there are no publications, the publication count becomes meaningless. What becomes important is the author's message, both in its breadth and depth. For example, in true peer review, one could deposit ten reports with minor changes among them. However, he or she would have only one message to convey. This also means that researchers will no longer

have to recycle the same idea into more archival versions of the paper. There simply will be no value in doing so in true peer review.

- Some have pointed out the incremental, often poor quality of work that the publish or perish paradigm induces. As a solution, their proposal is to make people understand that science happens slowly [8]. I don't think the problem is one of speed. The fundamental problem is gatekeeping. If that is fixed, the problem of incremental, poor quality work will disappear.
- True peer review will promote a more open community of researchers, one where researchers discuss and explore rather than write quick and often unduly harsh or superficial reviews under time duress as currently happens.
- In the pursuit of publications, it is *teaching* that has been getting the short shrift. Junior appointments are worried about having too high a teaching load because that would get in the way of publishing. At some institutions, faculty members are able to trade grant money for reduced teaching loads. At others, teaching is delegated to postdoctoral students and sometimes teaching assistants. Universities know the value of publications, so they help faculty members with reduced teaching loads. If the publication count were to become meaningless, then teaching would once again rise to the prominence it deserves. Teaching is *no less important* than research. Researchers who consider it of secondary importance do so at their own peril. In their students, they have a fresh readily available audience for their ideas—day in, day out, nine months a year. Among their students will most likely be the people who will in the future take their ideas and research program forward.

VI. CONCLUSIONS

What I have tried to show in this paper is that traditional peer review has almost nothing going for it except tradition whereas true peer review has no foreseeable flaws. Traditional peer review conflates the notion of peer review with the idea of gatekeeping. Gatekeeping is not an empirically validated activity; it just happens to be the traditional system. For those who argue that anything that replaces gatekeeping should be better, the onus is on them to first show how well gatekeeping performs.

If we go by the responses to PRC's survey [10], it seems that a majority of researchers find merit in peer review. Among the benefits cited are include improved quality, detection of fraud, and so on. Each of the cited benefits is potentially attainable to a greater degree in true peer review but without all the hassles and pretensions that are associated with the former.

Getting rid of gatekeeping means giving more importance to content, which should have been our guiding value, but was instead lost in the clamor for more and more publications. It is disheartening when researchers would rather work on finishing and submitting an unpromising paper given an impending deadline rather than think and talk about the underlying challenges. It is disheartening that our doctoral students are

so burdened with producing papers that they would rather not make any presentations in weekly seminars. It is disheartening to see them stumbling about in the dark, doing this and that, but never really striving to get to the crux of the matter. It's not their fault: one doesn't have to get to the crux of things to get published. It is disheartening to see that researchers are actually turned off by spirited but definitely polite debate. It is disheartening that we have set up a system which discourages the pursuit of knowledge.

If we get rid of gatekeeping, we will have to build *everything* anew. Because right now, everything is built upon the idea that gatekeeping has merit in that only the papers it selects have merit. Getting rid of gatekeeping means substantial changes in *evaluations*. It would affect the way we hire and promote researchers, allocate funding, and award honors. Here I would like to emphasize one thing that I already addressed in some detail in Section IV. One argument people bring up again and again is that true peer review is a pipe dream unless I can also show that systems of evaluations will also work better in a true peer review world. It would be good if I had those answers but the legitimacy of true peer review does not rest upon answering those questions. Conceptually, I see that any system of evaluations would be *built upon* an underlying system whose value is the pursuit of knowledge. The first thing is to make sure that the underlying system works in and of itself because if that system fails, then as computer scientists know well enough, everything built on top will fail. I have argued that current peer review fails as this underlying layer whereas true peer review succeeds.

Perhaps in the Internet-less age, gatekeeping served a purpose given the practical limits on dissemination. Now it hampers dissemination. It shackles researchers and science. Gatekeeping is nothing but an exercise in futility, vanity, and censorship. Let's get rid of it.

I think of each mind as a rich world of its own. And I like to think of true peer review as exploring a problem not only with your own mind but also through the minds of others. Knowledge is not out there; it is hidden deep inside the pathways of our minds. We can begin to get to it only by talking, discussing, and reflecting. The richness of the ideas that would be born from exploring many minds would be truly breathtaking.

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